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ELMER D. MERRILL
1876 - 1956

On the 26th February a protacted illness put an end to the life of Dr. Elmer D. Merrill. In a subsequent issue of Taxon a detailed account of his life and work will be given, and for this reason I will confine myself here to a few words by which I wish to express the high esteem I myself felt for this nestor of plant taxonomy.

Nearly thirty years have already passed since the day that I, then a young member on the Staff of the Utrecht Herbarium, had the privilege to be introduced to Dr. Merrill, who at that time paid a visit to our Institute. The unpretentious way in which my so much older and more experienced colleague discussed with me all kinds of problems, and in which he drew my attention to various peculiarities of the plants he was studying in our herbarium, made an unforgettable impression on my mind. Since then I have met him again and again, and at several occasions I have stayed for a somewhat longer time in his company than at our first introduction, and each time I was once more struck by the broadness of his views and warmed by his enthusiasm for our enticing science, which is so eminently suited to create international friendships. At many meetings I have seen him in the chair, and I always recognized in him the perfect leader. Quiet, certain of his grasp of the subject, and at the same time full of that sympathetic humour which is so essential to a good chairman, he knew how to prevent digressions and to obtain useful decisions without losing too much time.

As members of the I.A.P.T. and as officials of the International Bureau for Plant Taxonomy we will not forget that Dr. Merrill has been one of the first to recognize the desirability of organizing international coöperation in the field of Plant Taxonomy. In my attempts to give these ideas a practical form I have always found in him a staunch and stimulating supporter.

Dr. Merrill's death did not come unexpected. He reached a fairly high age and he was able to produce a considerable amount of scientific work of the highest order. Nevertheless, we will feel his absence very strongly, and the prominent place he occupied in our midst will not so soon be filled. The memory of this great plant taxonomist will ever remain alive with us.

J. LANJOUW.

INDEX MUSCORUM

The necessity for a new Index Bryologicus was discussed in a paper published in Taxon 3: 97. 1954. We are pleased to report that work on this project has now been commenced and we invite all bryologists to co-operate in the compilation of the required data. It is over fifty years since the publication of the 2nd edition of Paris' Index Bryologicus and during this period there has been a considerable output of bryological literature. At the International Botanical Congress at Amsterdam (1935) it was agreed to accept Hedwig's Species Muscorum as the starting point for moss nomenclature (Sphagna excepted) and the date of publication was arbitrarily fixed as December 31st (Stockholm 1950). The section Bryology of the Eighth International Botanical Congress (Paris 1954) devoted a session to the discussion of the urgent need for a new index which would (1) list additional information necessitated by the introduction of the type method in taxonomy (2) bring the nomenclature into line with the current International Code (3) add all the names published after the closing date of Paris' Index (1900) (4) rectify the errors and omissions in that work.

Prof. R. van der Wijk (Groningen) and Mr. P. A. Florschütz (Utrecht) agreed to act as the editors. Being the principal collaborator, Mr. W. D. Margadant (Groningen) afterwards joined the editorship. To facilitate the execution of the work on an international level an advisory committee was formed: Dr. V. Allorge, France; Dr. F. M. H. Demaret, Belgium; Prof. G. Sayre, U.S.A.; Prof. W. C. Steere, U.S.A.; Miss J. Taylor, U.S.A. Financial grants were received from the Netherlands Organisation for Pure Research (Z.W.O.), the Hague, and the National Science Foundation, Washington; the International Association for Plant Taxonomy has agreed to publish the work in the series Regnum Vegetabile.

The Index Muscorum will list in alphabetical order all genera, species and varieties of mosses (Musci) with the following references as far as possible:

a. name of genus with author, place, date of publication and type species.
b. name of species with author, place, and date of publication.
c. obligate (nomenclatural or homotypical) synonyms, i.e. synonyms based on the same type.
d. facultative (taxonomic or heterotypical) synonyms with reference to the author who first published the synonymy.
e. a rough indication of the geographical distribution.
f. subgenera, subspecies and varieties are treated in the same way; lower categories such as sub-varieties and forms are not included.

In this way the new Index will be totally different from the Index Bryologicus of Paris, and it will appear under a new name: Index Muscorum.

1. Bryologists are asked to co-operate by sending reprints of papers with new moss taxa or with references to synonymy or to nomenclature to the editor in chief, Prof. R. van der Wijk (papers published later than 1900 are especially welcome). If reprints are not available, the author should list the title, the periodical and the date of his paper as well as the new names and combinations published in them.
2. Bryologists are invited to let the editors know what geographical area and which group are covered by their private card indexes and to inform them whether they would eventually be prepared to send their indexes on loan. No indexes should be sent to the editors until the latter have sent a request for a loan. At the Paris Congress Prof. Y. Y. Horikawa promised to send his card index of Asiatic mosses; Prof. W. C. Steere and Dr. F. Demaret did the same for America and the Belgian Congo.

3. All bryologists can be of great help by answering questions or inquiries from the editors on papers not available in the Netherlands or on other special items.
4. Corrections of errors in Paris' Index Bryologicus ed. 2, or in Engler and Prantl, Pflanzenfamilien (Brotherus) ed. 2. 10 and 11 are especially welcome.

We feel confident that all bryologists both professional and amateur will welcome this work. We hope to produce an up to date index before the ninth International Botanical Congress at Montreal in 1959. All correspondence should be addressed to the undersigned, Botanical Laboratory, Department of Systematics, Groningen, Netherlands.

R. VAN DER WIJK.

THE SUBDIVISION OF THE POLYPODIACEAE

by

A. H. G. Alston (London)

In preparing an account of the ferns for the Flora of West Tropical Africa, an attempt was made at first, to follow the classification proposed by Holttum but there are several points about which the writer does not agree with him and in effect he is, therefore, obliged to propose a new classification. First those ferns with hairy and not scaly rhizomes have been segregated. Some are placed by Holttum in the *Dennstaedtioideae* and others in the *Pteridioideae*. They have creeping rhizomes and usually several bundles in the stipes. The spores are mostly trilete but those of *Hypolepis*, *Lonchitis* and *Histiopteris* are monolete. Paraphyses are often present and are of the jointed type. The annulus is slightly oblique, and *Lophosoria* in the *Protocyatheaceae* is perhaps ancestral; it has the same jointed hairs and similar paraphyses. The creeping rhizomes may not be primitive as sporeling plants of *Pteridium* appear to have tufted stipes.

There are other ferns with hairs instead of scales, among these *Cheiropleuria* should, in the writer's opinion, form a separate family (*Cheiropleuriaceae*). Some genera of the *Adiantaceae* have also hairs but these are not found in W. Africa. The *Protocyatheaceae* differ from the *Cyatheaceae* in this aspect.

The ferns with scales may be subdivided into groups on the basis of their spores: those with trilete and those with monolete spores. The latter group may again be divided into those with and those without a perispore. On the whole the spore characters seem to be reliable though there are genera with both types, as, for example, *Vittaria*.

The largest group among those with trilete spores is the *Adiantaceae*, which is the subfamily *Gymnogrammoideae* of Holttum. These ferns have sporangia with a short stalk, which appears to be a primitive character. They have no true indusia; but false indusia, produced by the specialisation of the leaf margin, are often present. The stipes is often rounded and black, and collenchyma is developed. They do have the pale lateral lines (pneumatophores) present as in many ferns. To this group I refer *Pteris* and *Acrostichum* which were placed in the *Pteridioideae* by Holttum. The *Vittaroideae* I have kept apart as *Vittariaceae*.

The genus *Acrostichum* seems to be rather an isolated type. It was placed with the *Pteridioideae* by Holttum and in the *Pteridaceae* by Copeland.

The trilete spores are like those of *Adiantaceae*. The reticulate venation without free veins like *Antrophyum* and *Syngramma*. The paraphyses resemble those of some species of *Antrophyum*. The stipes with many vascular bundles is, however, very different from the *Adiantaceae*, except for *Ceratopteris*.

Schizoloma and *Lindsaya* have also trilete spores, but have true indusia, directed outwards. It seems preferable to keep them in a separate family. The x shaped bundle of the stipes of *Lindsaya* is reminiscent of the structure of *Asplenium*.

The *Grammitidaceae* have long-stalked sporangia like those of the *Polypodiaceae* and *Davalliaceae*. Apart from the spores they may be distinguished from the *Polypodiaceae* by the fronds being adherent to the caudex and simple vascular structure. *Loxogramme* is somewhat intermediate between *Grammitidaceae* and *Polypodium* on account of the adherent fronds, though the spores appear to be either globose (and trilete) or oblong (and monolete), but the vascular structure of the stipe agrees with *Polypodiaceae*.

To the *Polypodiaceae* I have added *Stenochlaena*. This genus is placed in the *Pteridoideae* by Holttum and by Copeland in the *Blechnaceae*. The monolete spores without perispore suggest *Polypodiaceae* as do the peltate scales. Peltate scales seem to be found only in the *Polypodiaceae, Grammitidaceae* and *Davalliaceae*. Moreover the rhizomes tend to be glaucous as in some *Polypodiaceae*. The antheridia resemble those of *Drynariopsis* and not those of *Blechnum*. Finally the cytology agrees with that of the *Polypodiaceae*. The differences are the non-articulate fronds, yet in some species the pinnae are articulate, and the venation with parallel veins and a single row of areoles, but the latter is reminiscent of that of some species of *Goniophlebium*, which have a single row of areoles.

Holttum's subfamilies of *Dennstaedtiaceae* seem doubtfully related and it is more convenient to give them family rank.

His *Dennstaedtioideae* I have placed with *Pteridium* and its allies on account of the hairy rhizome and similar habit. The *Lindsayaceae* (his *Linsayoideae*) resemble *Adiantaceae* in their spores but the anatomy of the stipes is like that of *Asplenium*, with which *Diellia* seems a possible connecting link. The *Oleandroideae* and *Davallioideae* appear sufficiently close to be amalgamated as *Davalliaceae*. They have similar spores, sporangia and stipe-anatomy to the *Polypodiaceae*. The *Pteridioideae* I have divided between the *Dennstaedtiaceae*, *Adiantaceae* (*Pteris* and *Acrostichum*) and *Polypodiaceae* (*Stenochlaena*).

The *Thelypteridaceae*, which Holttum makes a separate family, and the *Athyrioideae* have a rather similar stipe-anatomy and monolete spores with perispore. The stipe-anatomy and apparently the absence of sclerenchymatous sheaths from the roots separates them from the *Dryopteroideae* and *Tectarioideae* which have similar spores. These two latter seem best amalgamated. It is hard to believe that such a plant as *Ctenitis clarkei* (Bak.) Ching in the *Tectarioideae* and *Dryopteris fibrillosa* (Clarke) Hand.-Mazz. can be far apart, when their outward appearance is so similar and both were once placed under *Dryopteris filix-mas* (L.) Schott.

It appears that the transition from hairs to scales, from trilete to monolete spores, from oblique to vertical annulus, from short to long-stalked sporangia may have occurred in more than one line of descent. The vascular structure and the development of a perispore may be more definite. The soral structure is evidently very plastic.

The attached table shows the arrangement of the W. African genera, to which have been added the basic chromosome numbers for the genera as given by Manton, though her work has not been based on W. African material. Those with an asterisk are unpublished and have been kindly supplied by Professor Manton.

a	Rhizomes hairy; spores mostly trilete, rarely monolete:			Anisosorus	49*-51*
	1 *Dennstaedtiaceae*			Lonchitis	
	Microlepia	86, 43		Histiopteris	32
	Hypolepis	52?	a*	Rhizomes scaly:	
	Pteridium	52		b Spores trilete; no perispore (in W. African species):	

c Indusium present:
2 *Lindsayaceae*
Schizoloma 44

c* No true indusium:
3 *Adiantaceae*
Ceratopteris 76-8
Acrostichum 30
Coniogramme 30
Pityrogramma 30
Adiantum 29, 30
Pteris 29
Notholaena 29*
Cheilanthes 29, 30
Doryopteris 30
Pellaea 29, 30
Aspidotis
Actiniopteris 29
4 *Vittariaceae*
Antrophyum 30
Vittaria 30
5 *Grammitidaceae*
Xiphopteris 37

b* Spores monolete; no perispore; eremobryoid.
c No true indusium:
6 *Polypodiaceae*
Platycerium 37
Pyrrosia 37
Stenochlaena 37?
Drynaria 37
Belvisia 35*
Loxogramme 36?
Phymatodes 36
Pleopeltis 35, 36, 37
Microgramma
Microsorium 36

c* Indusium usually present:
7 *Davalliaceae*
Nephrolepis 41
Arthropteris 41
Oleandra 41
Davallia 40

b** Spores with perispore; desmobryoid; indusia usually present:
8 *Aspleniaceae*
Asplenium 36
9 *Thelypteridaceae*
Thelypteris 31
Cyclosorus 36
Ampelopteris 36
10 *Athyriaceae*
Athyrium 40
Diplazium 41
11 *Aspidaceae*
Didymochlaena 41
Dryopteris 41
Polystichum 41
Ctenitis 41
Tectaria 40
12 *Lomariopsidaceae*
Elaphoglossum 41
Lomariopsis
Bolbitis 41
13 *Blechnaceae*
Blechnum 34, 33, 32, 28

As *Athyriaceae* is a new name for a family it requires a latin description.

Filices terrestres rhizomatibus apicem versus paleolatis, paleis non cancellatis nec peltatis integris vel dentatis, dentibus 2-cellulatis; stipitibus fibris vascularibus duobus indutis superne confluentibus in more U; costis supra canaliculatis, numquam pilis septatis indutis; soris curvatis vel punctatis, indusiis plerumque curvatis, rariter desinentibus; sporis monoletis perisporia ferentibus.

Type genus: *Athyrium* L.

In addition to the West African genera I refer *Matteucia* Tod. (N. = 40), *Woodsia* R. Br. (N. = 41), *Stenolepia* v. A. v. R., *Cheilanthopsis* Hieron. and *Cystopteris* Bernh. (N. = 42), to this family.

The family *Lomariopsidaceae* (the *Lomariopsidoideae* of Holttum) also requires a latin diagnosis.

Filices scandentes, epiphyticae vel rariter terrestres, rhizomatis paleaceis, paleis non peltatis; stipitibus supra basin articulatis vel adherentibus, fibris vascularibus numerosis; pilis simplicibus numquam septatis; soris foliorum superficies inferiores omnes tegentibus.

Type genus: *Lomariopsis* Fée.

The typification of the generic name Bambusa and the status of the name Arundo bambos L.

by

R. E. Holttum

The genus *Bambusa* was proposed by Schreber in 1789 (Gen. Pl. 1: 236), without citation of a species or any earlier synonym. In the addenda at the end of his second volume, Schreber added a reference to *Bambos* Retz. (Obs. Bot. 5: 24. 1789), but again did not mention a species. Retzius described only one species, *Bambos arundinacea,* which must therefore be the type species of Schreber's genus. Both Bambos and Bambusa were published in the same year. It is customary to regard Bambusa as having priority, but, if this should prove to be wrong, the type species remains the same.

Unfortunately there is no specimen of *B. arundinacea* in Retzius's herbarium at Lund (Fischer in Kew Bull. 1932: 76). Retzius cited as synonym *Arundo bambos* Auctorum, but not *Arundo bambos* L. McClure has discussed in detail both Retzius's description and other evidence about the identity of the specimen which he described (Blumea, Suppl. III: 97, 100, 101) and concludes that the specimen described by Retzius probably belonged to the same species as that later named *Bambusa vulgaris* by Schrader (in Wendl., Collect. Pl. 2: 26. *t. 47.* 1810). I agree with the conclusion. (Part of the evidence is that Retzius stated that the specimen had been sent to him by Koenig, and there is in the British Museum a specimen from Koenig's herbarium named *Arundo bambos* which is certainly *Bambusa vulgaris* Schrad. I have seen this specimen).

The next author to use Retzius's name was Roxburgh, who interpreted the name as referring to a large thorny bamboo (Pl. Corom. 1: 56. *t. 79.* 1798). When Willdenow transferred Retzius's name to *Bambusa* as *Bambusa arundinacea* in 1799, he stated that he had seen a specimen (Sp. Pl. 2: 245); the specimen so named in Willdenow's herbarium at Berlin was examined by McClure and found to be *Bambusa vulgaris* (I have seen a photograph of it). The name *arundinacea* however has continued in use by almost all authors to the present day for the common thorny bamboo of India; and the name *B. vulgaris* has universally been used since 1810 for the species which Schrader described, which is one of the commonest and also one of the most easily recognized of all bamboos.

If we typify the generic name *Bambusa* by the species *Bambos arundinacea* Retz., and if we consider that Retzius was describing the same species as Schrader, we have to make Schrader's name a synonym, and change the name *B. vulgaris* to *B. arundinacea;* in other words, we would alter the meaning of two names which have been widely and consistently used for nearly 150 years. This would indeed cause a confusion, and we cannot justify the change by referring to a type specimen. Because of this, McClure did not make the change of name; but he also did not propose any other solution of the difficulty.

Though the name *Arundo bambos* L. is not mentioned formally as a synonym of *Bambusa vulgaris* Schrad., Wendland stated (when publishing Schrader's description in his Collect. Pl. 2: 26) that he considered the new species to be that called *A. bambos* by earlier authors. The typification of *A. bambos* L. therefore needs to be considered, and this question has also been discussed by McClure. There is no description of the species in the first edition of Species Plantarum; we are therefore obliged to interpret *A. bambos* by reference to the previous descriptions cited. (McClure has shown that a specimen in the Linnean herbarium is later, and is to be identified as *Bambusa flexuosa* Munro, probably sent to Linnaeus by Osbeck from China). Linnaeus's references are as follows.

Arundo arbor.

(1). Bauhin, Pinax 18. This is too vague and general to be identifiable with a particular species.

(2). Hort. Cliff. 25. There is no description, but there is a specimen in the British Museum (a leafy branch only) that is almost certainly *B. vulgaris* Schrad.

(3). Fl. Zeyl. 47. The brief description was copied from Burman, who in turn copied from Commelin; it contained the words *cortice spinoso*. There is no specimen in Hermann's herbarium at the British Museum, nor any drawing.

(4). Roy. Ludgb. 67. Royen quotes Commelin, and gives no original description; presumably he referred to a plant cultivated at Leiden, probably *B. vulgaris*, as no other bamboo of comparable size is known to have been in cultivation in Europe in the early 18th century.

Tabaxir & Mombu arbor. Bauhin hist. 1, p. 222.

I have not seen this reference, but it can hardly refer to a single clearly identifiable species.

Ily. Rheede, Hort. Malab. 1, p. 25, *t. 16.*

McClure discussed this in detail, and concluded that it is the reference giving the clearest indication of a definite species (he did not discuss the preceding references). But he admitted that "the shadows of at least two species known today are discernible in the caption to plate 16 (and in the plate itself, for that matter)". The whole plant shown on the plate has no sign of the low spreading branches so characteristic of the thorny bamboo, and no such branches are shown in the detailed drawings (as they are for example in Rumphius, Herb. Amb. 4, *t. 4*, not quoted by Linnaeus). Thorny branches are however mentioned in Rheede's description.

Buchanan-Hamilton discussed Ily in his Commentary on the *Hortus Malabaricus* (Trans. Linn. Soc. 13: 478—481. 1822). He considered it identical with the common bamboo known in Bengal as *Bheru*, and of this remarked "the spines are chiefly confined to the principal stem". Furthermore, he clearly did not understand Rumphius's drawing of the slender thorny branches above mentioned, doubting whether these branches could even belong to any bamboo; I conclude that he had never seen a thorny bamboo plant at close quarters, and therefore his discussion seems to me of little value. He considered Roxburgh's figure of *Bambos arundinacea* (Pl. Corom. 1, *t. 79*) to represent *Arundarbor vasaria* Rumph., though Roxburgh showed thorny branches in his plate, and Rumphius certainly did not.

Thus the only extant specimen referred to in any of the works cited by Linnaeus under *Arundo bambos* in 1753 is a sterile one from Clifford's garden, probably referable to *B. vulgaris;* and it is equally probable that this was the species which Royen had seen in cultivation. It is in the highest degree improbable that the common thorny bamboo of India was in cultivation in Europe in the 18th century, whereas we know that *B. vulgaris* was, on the evidence of Wendland, in his comments on Schrader's species above mentioned (1810). The only references to thorns in the works cited by Linnaeus are: (a) Commelin's phrase *cortice spinoso*, which I do not understand; it is certainly not descriptive of spreading slender thorny branches, and might refer to spine-like roots emerging from nodes just above ground level; and (b) the description of the thorny branches (not shown in the drawing) by Rheede. If we admit that Rheede intended to describe the thorny bamboo, Linnaeus's name refers to two species; of which we must choose one. If we regard the specimen in Hort. Cliff. as the type, then we must make *B. vulgaris* a synonym of *A. bambos*, and change its name accordingly; the confusion which would be so caused would be considerable, and even if the evidence were unquestionable, the change would be undesirable. I submit that it is equally unsatisfactory to regard *A. bambos* as

typifying the common thorny bamboo of India, as McClure has done. One thing is quite certain: Linnaeus had no definite species in mind when he proposed the name. In my opinion, therefore, the name *Arundo bambos* L. should be regarded as a *nomen confusum*.

When however we come to *Bambusa vulgaris* Schrad., we are on firm ground; there is no doubt whatever regarding the nature of the species Schrader described, and that species is still universally known by his name; it is moreover one of the commonest and most widespread of tropical bamboos. I submit that by far the simplest solution of the problem is to make the genus date from Schrader (1810), and to make *B. vulgaris* the type species of the genus. I therefore propose the *conservation* of the name *Bambusa* Schrader (1810) as against *Bambusa* Schreber (1789) and *Bambos* Retzius (1789). *)

The question of the name for the common thorny bamboo of India remains to be considered. McClure has proposed that it should be called *B. bambos* (L.) Voss., but, as above indicated, that is not a very satisfactory interpretation. The first description which unquestionably applies to this bamboo is that by Roxburgh under the name *Bambos arundinacea* Retz. If we believe he was wrong in using Retzius's name for this bamboo (i.e., that he misinterpreted the species) and that he was in fact describing a new species, we cannot use for it the name *Bambos arundinacea* Roxb., as that would be a later homonym. The next name which was unquestionably used for the species (in the broad sense of Gamble) is *Bambusa spinosa* Roxb. (ex Buch.-Ham. in Trans. Linn. Soc. 13: 480, excl. syn. 1822). The description of Buchanan-Hamilton is not satisfactory, but Roxburgh himself gave a good one (Flora Indica 2: 198. 1832) and there is an excellent coloured drawing from Roxburgh's collection at Kew. Roxburgh believed he was describing two different species (*B. arundinacea* and *B. spinosa*), both thorny bamboos, differing in minor characters (size of plant and number of florets in a spikelet). Kurz and Gamble, having seen a large number of plants in the field, united the two; I shall follow them, but shall use the name *B. spinosa* instead of *B. arundinacea*. If any future student of Indian bamboos should wish to regard them once more as separate species, I suggest that he should provide a new name for the form called *B. arundinacea* by Roxburgh; for such a purpose it would be most undesirable to revive the name *B. bambos*.

The type species of the genus Gigantochloa and some other notes on the genus

by

R. E. Holttum (Kew)

The generic name Gigantochloa was first used by Kurz, for some bamboos previously included in the genus Bambusa, in his list of the plants of Bangka (Tijdschr. Ned. Ind. 27: 226. 1864). Kurz gave no description either of genus or species, and the publication of the name Gigantochloa dates from Munro's Monograph of bamboos (Trans. Linn. Soc. 26: 123. 1868). Munro described only three species: *G. verticillata*, *G. atter* and *G. heterostachya*. The third of these is in my opinion a Bambusa (see Journ. Arn. Arb. 27: 341. 1946). We are therefore left with two species from which to choose the type species of the genus.

Munro's first species was based on *Bambusa verticillata* Willd., the type specimen of which was a flowering branchlet collected by Thunberg in Java (seen by Munro).

*) Proposal (no. 11) for the conservation of the generic name **Bambusa** Schrader in Wendland, Collect. Pl. 2 : 26. 1810.

Munro included in his species specimens named *G. maxima* by Kurz, and others also. Kurz objected to this procedure (Indian Forester 1: 344) and stated that Munro was uniting quite distinct bamboos under one species. He himself considered there were two distinct species, *G. maxima* Kurz and *G. robusta* Kurz, the former with two varieties. He doubted whether Willdenow's type of *G. verticillata* belonged to *G. maxima* or *G. robusta*, as Thunberg had recorded the local name Tring Atter for it, which name belongs to *G. atter*.

The general nature of the structure of the spikelet is remarkably constant throughout the genus Gigantochloa. Some species have spikelets of distinctive form, such as the large and very hairy ones of *G. scortechinii* Gamble, but in other cases species which are quite distinct vegetatively may have spikelets which are not sharply distinct (e.g., *G. ligulata* Gamble and *G. wrayi* Gamble). A specimen which consists of spikelets only (as the type of *Bambusa verticillata* Willd.) is therefore not satisfactory in this genus. Even if one admits as much vegetative diversity in a species as did Munro in *G. verticillata*, one cannot be satisfied with a type specimen which cannot be assigned with certainty to one of the vegetative forms. I submit therefore that the species *G. verticillata* (Willd.) Munro has not been satisfactorily typified; and that the whole problem of its typification is a difficult one, about which different people are unlikely to agree. My conclusion is that the species *G. verticillata* is not satisfactory as the type species of the genus Gigantochloa.

I submit therefore that the second species of Munro, *G. atter* Kurz (based on *Bambusa thouarsii* var. *atter* Hassk., Plant. Jav. Rar. 41) should be regarded as the type species of the genus. It appears to be a well-known and distinctive village bamboo in Java.

Backer included *G. atter* in *G. verticillata* (Handb. Fl. Jav. 2: 276), thus making the latter species even more comprehensive than Munro. This is a defensible procedure, if the floral characters of *G. atter* and *G. verticillata* are so much alike that they are not clearly distinguishable. But from a practical point of view, it is necessary to have descriptions by which the vegetative forms so included may be distinguished (it is not often practicable to identify a bamboo plant from its flowers), and I would agree with Kurz that it is better to regard these vegetative forms as distinct species.

It should be noted that Munro only saw herbarium specimens, whereas Kurz had seen the living plants. If Kurz had made the first descriptions, a large part of the present confusion in the assignment of names might have been avoided. Living plants of bamboos have in most cases to be identified by vegetative characters, especially of young culm-sheaths, which are rarely well represented in herbaria. Much of the difficulty of the typification of the older botanical names of bamboos is due to the fact that the distinctive characters of the culm-sheaths were unknown to the original authors. I consider that it is undesirable to base new bamboo species on flowering branches and leaves alone.

I believe that Gigantochloa is a very natural genus, and that it is quite closely related to Dendrocalamus, though Munro, Bentham and Gamble placed the two genera in separate sub-tribes (for evidence concerning this, see my paper of 1946 quoted above). Its natural distribution is mainly in the region of Burma, Thailand and Indo-China. In the north of the Malay Peninsula (near the Thailand border) there are several species, but as one travels southwards, one finds that all disappear except one, *G. scortechinii*, which extends to the southern end of the Main Range. I have seen no wild Gigantochloas in the State of Johore. But in southern Johore, near an old settlement, I have seen bamboos which come very near to *G. maxima* Kurz, a species I have not seen elsewhere in Malaya.

The Gigantochloas of Java are all ranked by Backer as introduced, and the distribution of the genus in the Malay Peninsula supports this conclusion. There

can be little doubt that the Gigantochloas of Java (and the species in southern Johore) are indications of the travels of men. They were carried along in men's migration, because they are among the most useful of bamboos (far more useful than the native Schizostachyum spp. of Java). These village plants, propagated vegetatively, are in fact clones selected by men because of their special qualities.

There is no doubt that for a proper understanding of the Gigantochloas of Java one must study this genus in the region of Burma, Thailand and Indo-China which is its home. My own limited study of the Gigantochloas in the State of Kedah, close to the border of Thailand, indicates that the problem is a complex one. In a few places in northern Kedah there are fairly extensive bamboo forests, and I have paid two visits to one of these. I tried to recognize distinct species, but found a considerable range of vegetative variation as regards size of culms, thickness of their walls, and in characters of culm-sheaths (ligule and auricle). I have recognized two variable species, *G. ligulata* Gamble and *G. latifolia* Ridley, but I am by no means sure that the characters I have used to separate them are constant, and think it likely that the whole population is a kind of hybrid-swarm. The plants flower sporadically without dying, and certainly produce some good seeds. Cross-pollination by wind would ensure the continual production of new variants, and there seems no limit to the life of an individual plant.

From such a variable natural population one can of course select what appear to be the most valuable plants for vegetative propagation, and I believe that the people who took Gigantochloas to Java did this; so that the constancy of character of Awi Gombong (*G. maxima*) in Java may be due to this fact, and it may be that it was originally selected in Burma or Indo-China from a mixed population. But of course it is also possible that some of the Gigantochloas now known in Java may have originated there by natural crossing of the original plants taken there from the Burma region. It is a remarkable fact that the best Gigantochloas of Java are hardly known in the Malay Peninsula.

CAMPBELLIA AFRICANA AND VERSIPELLIS VARIEGATA

by

Rolf Singer (Tucuman)

Donk (1) insists that *Merulius infundibuliformis* rather than *Campbellia africana* should be the type species of the genus *Campbellia*. Since the first selection was in favor of *C. africana*, Donk bases his dissent on two facts (a) that the type was even earlier indicated by Murrill (b) that *M. infundibuliformis* was sent in by the collector whose name was commemorated in the generic name; "labels and documents .... in the Kew Herbarium leave no doubt as to this species being the correct choice". In the case of *Versipellis* Quél. there was a first selection in favor of *V. variegata*. Donk bases his dissent on the grounds (statement c) that this selection does not fit the original description and (d) even if it fitted it, it is not eligible because *Versipellis* contains about the same species as *Xerocomus*, and therefore, the type species of the latter must also be that of the former; because of not having anticipated and agreed with this view, I (and implicitly René Maire) am criticized by Donk for adhering to a "very formal attitude", but "ignoring considerations of logic, moral obligations, and spirit of the rules". I wish to add, regarding this last statement, that, enjoying personal acquaintance with Dr. Donk, I do not assume that these and similar remarks by him are meant in an offensive manner, and the present clarification of the situation is not offered in a polemical spirit but rather in order

to bring up a question which might keep harrowing those concerned with the selection of lectotypes in future analogous cases, or with reexamining the validity of previous choices. What indeed has to be done once a lectotype has been chosen illogically, immorally, and in contradiction with the (much quoted) spirit of the rules?

Ad (a): Murrill indicated types of genera according to the prescription of the American Code for Nomenclature, a "mechanical system" based on the "automatic selection of the first species or specimen cited or of a specimen collected by the person after whom a species was named", which "should be avoided as unscientific and productive of possible future confusion and further change" (2, App. I, 4, p. 49, 1952) .... "the lectotype should be selected so as to preserve current usage". "The first *choice* (italics mine) of a lectotype should be followed ...." (ibid., d and e, p. 50—51). I have already indicated that American Code generic types cannot possibly have the privilege of the priority in the sense of the Guide, Appendix I (see 6).

Ad (b): Although the first sentence quoted in the preceding paragraph refers to specimens being possible lectotypes of a species, those who invoke the spirit of the Rules should not insist on rejecting an already chosen lectotype in favor of another because the latter was collected by the person for whom the genus was named. But in absence of a formal ruling in a negative sense, there still is absolutely no indication in favor of considering at all such "intangibles" as seem to be in favor of *M. infundibuliformis.*

Ad (c): The villosity (as per diagnosis of *Versipellis*) is bound to appear, in a slightly gelatinized cuticular layer, somewhat agglutinated, and consequently, the hyphal hairs making up this villosity will be fasciculate when drying out if resistent enough not to be plastered flat onto the surface as a false "cutis". If the trichodermial palisade is very dense, the drying out following a wet stage with slight viscidity leads to a granular to verrucose surface as in most species of *Xerocomus* and the section of *Boletus rubellus.* The fact that these species are sometimes somewhat viscid does not exclude them from the diagnosis of *Versipellis* Quél. There cannot be any possible objection on this count, which does not mean that Donk's argument is lacking in good faith. However, what is obvious for a specialist of a group, regarding range of variability of a given character, its relative significance in descriptions of a given period and author, and the systematic consequences of proposed changes in lectotypes, is not necessarily obvious to anyone who approaches the problems with the bookish meticulosity of an O. Kuntze. Therefore, it has been recommended (ibid.) that "designation of a lectotype should be undertaken only in the light of an understanding of the group concerned".

Ad (d): Here, we enter again the "intangibles", assembled with what we have termed "bookish meticulosity", not with any ill meaning of the word, but in contrast to the tangible argument based on the letter of the rules and recommendations and on the practical needs of the working taxonomist. It appears that anybody suggesting a lectotype because of the wish to eliminate an embarrassing name, or to fortify a generally accepted one — in other words, applying the rules, created "to aim at fixity of names", (Art. 4) and according to which "established custom must be followed" (Art. 5), and those guiding principles that tend to preserve "current usage" (App. I, 4 d) — is accused of "juggling", a word we had all thought to be extinct since the last of Lloyd's notes. And "juggling" seems to be contrary, in Donk's dictionary, to the moral obligations and the spirit of the rules. Now this spirit of the rules is nothing intangible at all but is firmly anchored in the introductory articles 1—5, and it is recommended to reread Art. 4 and 5 in order to establish that the opposite of a ruthless dogmatism in typification is concordant with the

spirit of the rules, and that "other considerations .... are relatively secondary". Consequently the only moral obligation of anybody undertaking typification is to obey the rules and recommendations, the latter within the spirit of the rules, and with a sound balancing of judgment wherever such recommendations may contradict each other in a given case as may happen in the application of Appendix I, but does not, happen in the two cases we are now discussing. It may be that Donk synonymizes "moral obligations" and our supposed duty to give a "fair trial" (Donk, l.c. p. 310) even to the most embarrassing names. If this means scrupulous application of the Code, there could not be any disagreement. But if fairness is taken in its dictionary sense, I am unable to see how an empty set of letters representing a taxon can have the fair trial a human expects in court. A fair trial can be given only to persons. Although the rules explicitly underscore the relatively unimportant rôle of "regard to persons" (Art. 4) or of the person of the collector (App. I 4), it is only just to investigate whether any persons involved in the typification or publication of *Versipellis* and *Xerocomus* were unfairly treated. Typification was proposed in form of a lectotype selection by Singer. Singer was hardly unfair to Singer. Publication of the taxa cited is due to Quélet. Quélet wished unequivocally and emphatically for us to use the later generic name *Xerocomus* rather than *Versipellis*. By choosing a lectotype in accordance with that wish, unfairness was not manifest. And in regard to the taxonomists who at all use a generic or subgeneric unit corresponding to *Xerocomus*, it may be said that all without a single exception follow Quélet's wish; not one has adopted *Versipellis*.

Logic was another term which, according to Donk supports his point of view. Does this mean that the guide is worded so as to be termed illogical? I do not find any evidence that it is nor that Donk means that it is. Logic is then to be used in applying the rules and recommendations so as not to contradict the laws of logic. It does not seem illogical to assume that a genus A which has essentially the same scope as a genus B must not necessarily have the same type species. Assuming that the common author of the two genera had indicated a type different in each case, to both A and B — we would be obliged by the rules to respect this original designation. Why, then, in the name of logic, would it be illogical to admit the same possibility when the type has to be selected? Only, according to the rules, if one name were the basionym of the other, or if both were based on the same basionym, which is obviously not the case in our example. However, this, according to Donk, is "a very formal attitude", even "an abuse of the type method".

It may actually seem to the superficial observer that Donk's entire tendency is a conscious attempt to attack stability of nomenclature by accumulating intangible arguments for or against a lectotype. This, however, is not so. Donk believes that the generic names killed in this manner can be easily revived by voting them back as nomina conservanda. I am not the only one who has tried to convince Donk that both his arguments marginal to or outside the actual Code text as well as his hope for a long list of nomina conservanda to be voted conveniently in order to avoid the practical consequences of exaggerated purism in typification are strictly Donk's own philosophy of Botanical Nomenclature. In vain.

There is however a case where Donk himself rejects the idea of a deus ex machina in form of a nomen conservandum, and still attacks a lectotype interpretation that would tend to clarify the genus *Gyrodon*, one of those genera that is almost generally accepted by mycologists excepting a very few who do not even introduce subgeneric names for a genus *Boletus* sensu lato. I must admit that in this case, Donk's reasoning remains entirely obscure to me, and I merely mention the details in order to show that Donk's purism is not always compensated for by the abortive attempt to remedy the situation afterwards. The type of *Gyrodon* was, as Donk indicates, determined

as *G. sistotremoides* = *sistotrema* Fr., and by one of the rare lucky instances where old European species can still be traced, I had shown that a specimen exists at Uppsala that was determined by E. M. Fries himself. This authentic determination, in addition to the fact that there is no contradiction between it and the original description, was rejected by Donk as inconclusive (thus making *Gyrodon* a supposed *nomen dubium*) on the grounds that the specimen is not the holotype, and that Fries never consciously admitted the conspecificity of *Gyrodon sistotrema* and *Boletus lividus*. Any formal scruples regarding the type specimen can be remedied by the selection of the specimen in question (5) as neotype of *B. sistotrema* and *sistotremoides* which is herewith proposed. The other argument remains completely obscure since it cannot be demanded that a species is a synonym of another species only if the author of the synonymous species consciously admits the synonymy. My paragraph (4) quoted by Donk and written before *G. sistotrema* was cleared up (5) is a straightforeward and factual discussion of the situation, and any construction of a difference in the characters of *Gyrodon lividus* and the *description* of *G. sistotrema* is in contrast to the knowledge of any bolete specialist (cf. the description of *G. lividus* in Kühner & Romagnesi, 2), and Kallenbach as quoted by Donk does not deny the possibility of an agreement of the characters of the type species with those of *G. lividus* but merely cites Fries suspicion — not justified as shown later on — that *G. sistotrema* might be merely a deformed *Boletus*. Under the circumstances, it does not seem to be acceptable when Donk either "rejects" or corroborates a specialist's conclusion who has studied the genus and the species monographically. The term "ostrich policy" does not apply to such a specialist but rather to those who throw the existing evidence overboard in a case that could not possibly be more clearcut and less open to divergent interpretations.

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ON THE OCCURRENCE OF CYANOGENETIC GLYCOSIDES IN THE ORDER RHOEADALES

by

J. M. Honeyman (Montreal)

During the past two years the writer has carried out tests on species of plants belonging to the Order Rhoeadales as delimited by Harms (1) for cyanogenetic glycosides as indicated by the production of hydrogen cyanide. The intention was to discover if any relationship existed between the presence of these compounds and the taxonomic position of the species.

The testing procedure is well known and recently has been described and modified by Gibbs (2). The plant material, ground and mixed with emulsin, was placed in a test tube from the glass stopper of which was suspended a strip of sodium picrate paper. A colour change from yellow to brick-red indicates the presence of hydrogen cyanide. In most cases seeds were used but fresh leaves were tested also.

In view of the fact that a group of Danish workers (3) have studied the distribution

of isoThiocyanates in this order and that a group from California recently have signified their intention of doing so in this journal (4), it may be of interest to summarize here the results of these hydrogen cyanide tests.

In the following table the first figure refers to the number of genera involved, the figure in brackets to the number of species tested. In the case of the Cruciferae the tabulation has been made in relation to the Prantl (5) subdivisions of the family.

ORDER RHOEADALES

	HCN Positive	*HCN Negative*
Cruciferae		
Thelypodieae	1(1)[a]	3(3)
Sinapeae	9(11)[b]	33(88)[c]
Schizopetalae	—	1(1)
Hesperideae	1(1)[d]	29(57)
Papaveraceae		
Hypecoideae	—	1(3)
Papaveroideae	1(1)[e]	9(33)
Fumarioideae	—	3(9)
Capparidaceae	—	1(2)
Koeberlineaceae	—	1(1)
Moringaceae	—	1(1)
Resedaceae	—	1(4)
Tovariaceae	—	1(2)

a. *Heliophila amplexicaulis.*
b. *Armoracia lapathifolia, Cakile edentula, C. edentula var. lacustra, Cardamine dictyosperma, Eruca sativa, Isatis djurdjurea, Lepidium graminifolium, L. sativum, Peltaria alliacea, Succowia balearica, Thlaspi arvense.*
c. Includes all 31 species and varieties of *Brassica* examined.
d. *Alyssum granatense.*
e. *Eschscholtzia californica.*

Because of the chemical similarity of the Tropaeolaceae to the Rhoeadalian families in the presence of isoThiocyanates, three species of *Tropaeolum* were tested but found to lack hydrogen cyanide.

Though the presence or absence of cyanogenetic glycosides cannot in itself be said to be a striking diagnostic character yet in combination with the results of other chemical analyses it may assist in the solution of certain taxonomic problems.

The writer would appreciate hearing from anyone having plant material of *Bretschneidera* or other less commonly available taxa.

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THE INTERNATIONAL RULES OF ZOOLOGICAL NOMENCLATURE*

by

R. E. Buchanan (Ames, Iowa)

The XIV International Congress of Zoology held in Copenhagen, 1953, approved the content of the Revised International Rules of Zoological Nomenclature, and directed that they be edited for publication. The literature bearing upon the revision of the "Rules" is voluminous and detailed, it is phrased carefully and usually quite legalistically. The zoologists charged their International Commission on Zoological Nomenclature with the task of preparing draft copy for the revision to be finally approved at the Copenhagen Congress of 1953. This Commission held fifteen meetings in Paris in July, 1948, at the time of the XIII International Congress. Practically the entire Volume 4 (parts 1-24, 1950) of their official organ *"The Bulletin of Zoological Nomenclature"* is given over to the "Official Record of Proceedings of the International Commission on Zoological Nomenclature". Including the excellent index, there are 760 pages. On December 31, 1953, there was published the "Copenhagen Decisions on Zoological Nomenclature of the Règles Internationales de la Nomenclature Zoologique" adopted by the Copenhagen Congress, August, 1953, and edited by Mr. Francis Hemming. At this Congress Mr. Hemming was authorized to proceed with the preparation of the Official Records of the Copenhagen meetings and Prof. J. Chester Bradley, President of the International Commission was invited to prepare the first draft of the revised rules. These rules have not yet (Jan. 15, 1956) been published, so they are not available in final form.

The meaning of the revised rules may be gleaned from the publications noted above, but the pertinent statements are scattered through fifteen hundred pages or more. Fortunately, Dr. W. I. Follett, of the California Academy of Science, has prepared, and the Society of Systematic Zoology in the United States of America has printed and distributed a most helpful brochure. Because of the restrictions placed by the International Zoological Trust upon publication of quotations from their copyrighted publication, the brochure includes in its title "An unofficial interpretation of the International Rules of Zoological Nomenclature" and the cover bears the statement "Issued September 1955. Not published." The booklet (100 pp.) is paraphrased from the "Bulletin of Zoological Nomenclature" and the "Copenhagen Decisions". The material is well organized, includes a glossary, and has an excellent index. The author states clearly that: "it should not be quoted as an authority for any nomenclatural proposition".

The Glossary is of interest in showing the development of a terminology which has come to differ in many respects from that of Botany and Bacteriology. These differences in use of terms are particularly significant to the microbiologist working with the protozoa, and are of interest to all biologists. Some of the differences are noteworthy:

Binary nomenclature (the binomial nomenclature of the bacteriologist) has been replaced by *"binominal nomenclature"*.

Binomen is introduced as the standard expression to indicate the name of a species, replacing the older designations, species name and specific name. It is the binominal designation which constitutes the scientific name of a species. The wisdom of this rechristening of the species name may well be debated. There is no classic Latin word *binomen*, but the adjective *binominis* is defined as "having two names". One is puzzled to find that the name of a species has two *names*.

Specific name. The Paris (1948) Zoological Congress defined "specific name" as the scientific name of a species. The Copenhagen (1953) Congress as noted above replaced the phrase "specific name" by "binomen". It then redefined "specific name" as the "name" that distinguishes a "nominal species" from the other species of that "nominal" genus. In other words, a specific name is the designation of the second component of the name of a species. The term used for this in bacteriology and botany is *specific epithet*. The terms *specific name* and *species name* are not synonymous in

*) This article is reproduced from the International Bulletin of Bacteriological Nomenclature and Taxonomy 6(1) : 43-46. 15 Jan. 1956.

zoology as they are in bacteriology. Most specific epithets (specific names in zoology) are adjectives: there is some element of anomaly in terming a descriptive adjective such as "*aureus*" a name. There is merit in the appellation of "*epithet*".

Trivial name. The designation of the second component of the name of a species was fixed by the Paris Zoological Congress as the "trivial name". This was discarded by the Copenhagen Congress and replaced by "specific name". The term "trivial name" in zoology is a synonym of "specific epithet" in bacteriology. This lack of uniformity in terminology, the use of the same term with different meanings in the several branches of biology, is most confusing and unfortunate. Apparently there has been no effort among the several branches of biology to cooperate in eliminating discordant terminology.

Trinomen is the designation of the name of a subspecies, consisting of the name of the genus followed by the "specific name" (specific epithet) and this in turn by the "subspecific name" (subspecific epithet). The corresponding term in bacteriology is subspecific or subspecies name.

Subspecies is defined as a population (e.g., geographic or ecological) within a species that is to be distinguished from any other such population within the same species.

Opinion and *declaration* are distinguished. An *opinion* is a decision by the Commission having to do with the status of a name or book. A declaration is a decision by the Commission which interprets a rule. In the Bacteriological Code both are included in "Opinions".

Infrasubspecific form. A term used to designate any form (subdivision) of a species, other than a subspecies, such as seasonal, sexual and transitional forms, aberrations and minority elements of all kinds within a species. In spite of its length, *infrasubspecific* may be useful as an over-all term to include in bacteriology the subdivisions of species such as serotype, strain, stage, group, phase, and others.

Nominal. The Zoological Rules recognize the use of the term genus in two senses. In a general sense *genus* is the name applied to that taxon made up of related species, it designates a definite taxonomic category. A *nominal genus* is a particular genus which has been characterized and to which a distinctive name has been given. Experience has shown that when used in the rules and recommendations of a code the two related but distinct usages of the word genus should be distinguished. This distinction will be made in the Zoological Rules when published. Similarly, *species* is the name given to a taxon of a definite rank; a *nominal species* is one which has been defined and named.

Homonym. A homonym is defined as the same name applied to two or more different units of the same taxonomic category (genus or species); the name of each is a homonym of the other. When two nominal species are given the same name (binomen) in the same original publication, the binomina (species names) are *primary homonyms.* When two nominal species, at the time of the publication of their respective names, are placed in different nominal genera and are given the same specific name (specific epithet), and are subsequently placed in the same nominal genus, each binomen (species name) is a *secondary homonym* of the other.

Clearly many of the definitions and new terminologies to be included in the revised Zoological Rules may well be considered in revision and interpretation of the rules and recommendations of the Bacteriological Code. One may, however, hesitate to accept the dictum that an adjective (*albus, achrous*) or a noun in the genitive (*enteriditis, suis*) which appears as the second component of the name of a (nominal) species is to be regarded as a *name,* as in the expression "specific name" approved for use in zoology. There are some manifest advantages in the continued use by bacteriologists of the grammatically correct phrase, *specific epithet.*

One may note some inaccuracies in the statements to be included in the revised Zoological Rules. In an appendix termed the "Second Schedule" there is given a table which "indicates the manner in which Greek words should be transliterated". The table includes several errors. In most cases there is no exact transliteration of the Greek word used as an illustration. As an example of an error, the Latin equivalent of the Greek *omega* (ω) is given as oe. It should be long o (ō).

INDEX NOMINUM GENERICORUM

Bulletin no. 3

Dr J. J. Swart

The Netherlands Organisation for Pure Scientific Research (Z.W.O.) has decided to give a special grant to Dr J. J. Swart (Botanical Museum, Utrecht, Netherlands) to enable him to work 4 days a week on the *Index Nominum Genericorum.*

Dr J. J. Swart, whose chief interests are in the S. American *Burseraceae* and in botanical bibliography, had already prepared, in 1955, the texts for the cards of the *Rutaceae.* He is now going to prepare the texts for the following groups: *Sapindales, Rutales, Geraniales, Polygalales, Daphniphyllales* and *Melianthales.* In a few groups this field may overlap that of other collaborators; in such cases Dr Swart will of course take up contact with his collegues.

It goes without saying that the appointment of Dr Swart to the staff of the "International Bureau", with the special object to work on the I.N.G. is of considerable importance for a successful continuation of this work. The Executive Committee of I.A.P.T. is therefore greatly pleased to welcome him to his new task.

Second set.

A second set of 1000 cards is now being printed. Hitherto texts have been prepared and submitted by the following cooperators (the numbers are the code-numbers used on the cards to indicate the authors):

1. C. E. B. Bremekamp, Utrecht, Netherlands (*Acanthaceae, Pedaliaceae, Thunbergiaceae*).
2. P. C. Silva, Urbana, Ill., U.S.A. (*Algae*).
3. W. Rothmaler, Greifswald, Germany (C. European genera of *Phanerogamae*).
4. R. Bakhuizen van den Brink Jr., Leiden, Netherlands (Malaysian genera of *Phanerogamae*).
5. P. A. Florschütz, Utrecht, Netherlands (*Musci*).
6. G. F. Papenfuss, Berkeley, Cal., U.S.A. (*Algae*).
7. W. T. Stearn, London, Great Britain (*Myristicaceae, Martyniaceae*).
8. J. Taylor, Cincinnati, Ohio, U.S.A. (*Musci*).
9. R. E. G. Pichi-Sermolli, Florence, Italy (*Pteridophyta*).
10. J. J. Swart, Utrecht, Netherlands (*Rutaceae*).
11. F. A. Stafleu, Utrecht, Netherlands (*Phanerogamae*).
12. C. E. B. Bonner, Genève, Switzerland (*Hepaticae*).
13. H. Hara, Tokyo, Japan (Japanese genera of *Phanerogamae*).
14. L. B. Smith, Washington, D.C., U.S.A. (*Bromeliaceae*).
15. P. Leenhouts, Leiden, Netherlands (*Burseraceae* p.p.).
16. J. Schuurman, Bogor, Indonesia (*Crassulaceae, Saxifragaceae*).
17. R. D. Wood, Kingston, R.I., U.S.A. (*Characeae*).
18. L. M. Irvine, Cambridge, Great Britain (*Ceramiaceae*).
19. H. N. Andrews, St. Louis, Mo., U.S.A. (*Fossil plant genera*).
20. J. W. Thieret, Chicago, Ill., U.S.A. (*Scrophulariaceae*).
21. J. F. M. Cannon, London, Great Britain (*Umbelliferae*).

The second set of cards will contain texts of almost all collaborators cited above.

Rate of publication.

The first set of 1000 cards was published in October 1955; two or three sets will be published in 1956; three to five sets per year are to follow afterwards. It is anticipated that the complete Index will consist of about 40.000 cards.

The second set of cards will probably be issued in May or June 1956.

Arrangement.

Apart from alphabetically arranging the cards it will be of advantage to many users of the Index to have a second set arranged systematically in accordance with the names of the divisions and families mentioned on the cards. Already after the publication of the third set, several important groups of plants will be practically completely represented (e.g. *Acanthaceae, Bromeliaceae, Rutaceae,* conserved and rejected names of genera of algae, etc.).

Subscription.

All prices are per set of 1000 cards.

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Others $ 8.00 (£ 3.—.—; D. fl. 30.20)

*) Number of sets not limited.

News and Notes

NINTH INTERNATIONAL BOTANICAL CONGRESS

Montreal 1959

The Ninth International Botanical Congress will be held in Montreal, Canada, during the summer of 1959. Preparations for the Congress have been initiated but the exact date and officers of the Congress will be announced later. For the present all communications should be addressed to the Secretary of the Executive, Dr A. J. Skolko, Botany and Plant Pathology Division, Science Service Building, Carling Avenue, Ottawa, Ontario, Canada.

DONATION TO I.A.P.T.

The Kennicott Club of Chicago, an association of professional biologists, has made a donation of $ 25.— toward the publication of the journal Taxon.

The Executive Committee of the I.A.P.T. has gratefully accepted this donation and expressed its high appreciation of this sympathetic gesture.

BAUHINIA

The "Basler Botanische Gesellschaft", founded in 1952, is issuing a new Journal: "Bauhinia", mainly devoted to plant taxonomy. The journal will appear in yearly issues, the first of which was published on 31 December 1955 (Price Sw. frs. 18.—). The previously issued "Mitteilungen der Basler Botanischen Gesellschaft" (1.1, 1953 and 2.1, 1954, both Sw. Frs. 1.80) will not be continued.

The Basler Botanische Gesellschaft is anxious to enter into exchange of publications with other societies and institutes. Applications for such an exchange should be addressed to Mr P. Aellen, Mittlere Strasse 201, Basel, Switzerland.

COLOCYNTHIS MILL. (1754)

The name *Citrullus* Forsk. (1775) has been proposed for conservation for this genus and accepted by the Paris Congress. The result is that the earliest name for the water melon would now seem to be uncertain. *Citrullus* Forsk. was probably proposed for conservation with a view to excluding *Anguria* Mill. but since *Colocynthis* Mill. and *Anguria* Mill. were both published in the same work, in 1754, there seems nothing against retaining *Colocynthis*, which has been correctly used in several recent works. *Citrullus* Forsk. (1775) is, moreover, a *nomen nudum*: the first valid publication was by Schrader (1836). This conservation should be reconsidered with full knowledge of the facts.

Extract from A. W. Exell, Suppl. Cat. Vasc. Pl. S. Tomé : 21(1956).

COMMITTEE FOR SPERMATOPHYTA

The Committee for Spermatophyta has coopted some members whereas some other members have resigned. The present composition of the Committee is the following:

Secretary: H. W. Rickett, The New York Botanical Garden, Bronx Park, New York 58, N.Y., U.S.A.

Members:

B. Boivin, Ottawa, Canada.
A. A. Bullock, Kew, Great Britain.
J. do Amaral Franco, Lisboa, Portugal.
F. R. Fosberg, Washington, D.C., U.S.A.
B. P. G. Hochreutiner, Genève, Switzerland.
R. A. Howard, Cambridge, Mass., U.S.A.
A. R. Pinto da Silva, Sacavem, Portugal.
A. C. Smith, Washington, D.C., U.S.A.
C. G. G. J. van Steenis, Leiden, Netherlands.

PSEUDOTSUGA MENZIESII

versus

Pseudotsuga taxifolia

On p. 19 of Taxon, Febr. 1955, we referred to the article by Dr B. Boivin on the legitimacy of the name *Pseudotsuga taxifolia* (Poiret) Britton. We regret not to have mentioned at once that Dr J. do Amaral Franco, in the same number of the Boletim da Sociedade Broteriana (28 : 115. 1954) published an

answer to Dr. Boivin. Dr Franco points out that Desfontaines (1804) quoted the synonym *Pinus picea* L. and that consequently the name *Abies taxifolia* Desf. (1804) is validly published (although illegitimate of course). This means that it invalidates the later homonym *Abies taxifolia* Poiret (1805), the basionym of *Pseudotsuga taxifolia* (Poiret) Britton ex Sudw.

Dr Amaral Franco points out also that Mirbel (Hist. Nat. Pl. 10. 1804 or 1805) gives a detailed French description (p. 232) and a short Latin one (p. 241) of the European Silver Fir, both under the name *Abies taxifolia*, with the synonym *Pinus picea* L. If published before *Abies taxifolia* Poiret 1805, this publication would also make Poiret's name illegitimate.

The oldest legitimate name available for the species is *Abies menziesii* Mirbel (Mém. Mus. Paris 13 : 63, 70. 1825); under *Pseudotsuga* the legitimate combination is *P. menziesii* (Mirbel) Franco. Under the Code N. L. Britton, the author of the combination *Pseudotsuga taxifolia* ought to have adopted the *oldest available legitimate* epithet. This epithet is, as far as is known to us to-day, "*menziesii*", and under the present circumstances the combination *Pseudotsuga menziesii* (Mirbel) Franco is legitimate; *Pseudotsuga taxifolia* is illegitimate.

F. A. S.

NOTICES OF BOOKS

Contributions to this column will be gratefully received by Mr K. U. Kamer, Botanical Museum, Lange Nieuwstraat 106, Utrecht, Netherlands.

Flora of Madagascar

A. Cavaco & M. Keraudren: 58. *Santalaceae*; 59. *Olacaceae;* 59bis *Opiliaceae.* (all Paris, 1955).

Mosses of the Ucraine

A. S. Lazarenko: Opredelitel' listvennikh mkhov Ukrainy (Synopsis of the Mosses of the Ucraine), 2nd ed. Ed. Akademiya Nauk Ukrainskoy S.S.R., Kiyev, 1955. 466 pp., 77 figs., with keys and descriptions.

* * *

Monographs, revisions, etc., published in periodicals

R. S. Adamson: The South African species of *Lightfootia.* Journ. S. Afr. Botany 21 (4) : 155-218 (1955).

T. Amakawa & S. Hattori: A revision of the Japanese species of *Scapaniaceae* III. Journ. Hattori Bot. Lab. 14 : 71-90 (1955).

N. L. Bor: Notes on Asiatic Grasses 24. The genus *Arundinella* Raddi in India, Burma and Ceylon. Kew Bull. (3) : 377-414 (1955).

J. Cuatrecasas: El género *Mnioides.* Folia Biologica Andina 1 : 1-7 (1954).

Id.: Synopsis der Gattung *Loricaria* Wedd. Fedde Rep. 56 (2) : 149-172 (1954).

Id.: A new genus and other novelties in *Compositae.* Brittonia 8 (2) : 151-163 (1955). (Contains a synopsis of the new genus *Paragynoxys.*)

Id.: Taxonomic notes on neotropical trees. Tropical Woods 101 : 10-28 (1955). (Contains a synopsis of *Neosprucea.*)

B. H. Danser: Supplementary notes on the Santalaceous genera *Dendromyza* and *Cladomyza.* Nova Guinea (new ser.) 6 (2) : 261-277 (1955). (With keys to the species.)

W. H. Hodge: Flora of Dominica, B.W.I., part I. Lloydia 17 (1, 2, 3) : 3-238 (1954). (Covers Pteridophytes, Gymnosperms, and Monocotyledones, with discussion of factors of environment, agriculture, plant communities, botanical exploration, and bibliography.)

Ding Hou: A revision of the genus *Celastrus.* Ann. Mo. Bot. G. 42 (3) : 215-302 (1955).

T. Koyama: Taxonomic study of *Cyperaceae* in Eastern Asia 4. Acta Phytotax. & Geobot. (Kyoto) 16 (2) : 33-41 (1955). (Contains a revision of the East Asiatic species of *Kyllinga.*)

M. E. Mathias & L. Constance: The genus *Oreomyrrhis* (*Umbelliferae*), a problem in South Pacific distribution. Univ. Calif. Publ. Bot. 27 (6) : 347-416 (1955).

E. Nasir: The *Bupleura* (*Umbelliferae*) of North-West Himalaya. Univ. Calif. Publ. Bot. 27 (7) : 417-445 (1955).

E. Nelmes: Notes on *Cyperaceae* 28. *Scleria* Berg, Sect. *Hypoporum* (Nees) Endl. in Africa. Kew Bull. (3) : 415-453 (1955).

A. E. Porsild: The vascular plants of the Western Canadian Archipelago. Nat. Mus. Canada Bull. 135 (1955) IV + 226 pp. (Contains, a.o., an annotated enumeration of species.)

W. Rössler: Die Scleranthus-Arten Österreichs und seiner Nachbarländer. Österr. Bot. Zeitschr. 102 (1) : 30-72 (1955).

N. Takaki: Researches on the *Brachytheciaceae* of Japan and its adjacent areas I & II. Journ. Hattori Bot. Lab. 14 : 1-28; 15 : 1-69 (1955). (With a key to genera and revisions of several genera.)

P. Taylor: The genus *Anagallis* in tropical and South Africa. Kew Bull. (3) : 321-350 (1955).

H. C. de Wit: A revision of the genus *Cassia* (*Caesalp.*) as occurring in Malaysia. Webbia 11 : 197-292 (1955).

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"Taxon" is directed by: Prof. J. Lanjouw, Chief Editor, and Dr F. A. Stafleu, Assistant Editor. All correspondence should be sent to the International Bureau for Plant Taxonomy and Nomenclature, 106 Lange Nieuwstraat, Utrecht — Netherlands.
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